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ART. XXXII.—*On the Crumpling of the Earth's Crust;** by WILLIAM B. TAYLOR.

THE causes and conditions of the vast series of movements which have taken place throughout the earth's crust, present perhaps the most fundamental problem of physical or dynamical geology. And yet this problem is one of the most difficult and unsettled in the range of geological inquiry. It is indeed a very complex one, embracing several quite distinct features. The enormous lateral compressions which have everywhere crushed, folded, and contorted the stratified rocks, culminating in vast uptilted and fractured mountain ranges, may or may not have been instrumental in the elevation of continents, and the depression of ocean-beds. The alternate subsidence and emergence through miles of vertical oscillation of continental areas—successively repeated (by the agency of internal or external influences)—may or may not be correlated with equivalent upheavals and submergence of the present oceanic areas. And whether we infer with some geologists that ocean and continent have more than once changed places, or on the other hand conclude with Dana, and others, that the skeletons of the existing continents were rudely blocked out and the foundations of the great deep laid down in pre-archæan times, we are on either supposition beset with grave and peculiar alternative perplexities. The hypothesis of Joseph Le-

* Read before the Philosophical Society of Washington, May 23, 1885.

Conte, and others, that the tangential stresses of secular lateral compression have resulted in very great differences of thickening in the crust, deepening simultaneously the foundations of the mountains as they were slowly protruded upward, while leaving the great depressions proportionately attenuated, so that the internal topography of the shell is an obverse corresponding closely with its external contours,—although an hypothesis framed for the avoidance of supposed hydrostatical difficulties in the conception of a layer of approximately uniform thickness, floating on the liquid abyss,—is yet on both geological and physical grounds exposed to at least as weighty objections.

It is proposed in this communication to consider only the probable origin of that great contraction of the terrestrial crust so widely manifested in its plications or crumplings, increasingly marked as we approach the great oceans.

The liquidity of our globe, and the relative thinness of its incrusted envelope,—as attested by all legitimate *geological* induction,—will be assumed without misgiving or hesitancy; and the supposed mathematical arguments for its solidity—ignored as essentially fallacious and wholly inconclusive. It must be lamented that the professional mathematicians have as yet contributed nothing to the advancement or to the extension of geologic theory:—not from lack of analytic skill in a Thomson, or a Darwin, but from the want of proper data to justify the conclusions they have so boldly hazarded.

On the universal recognition from numerous evidences that our earth is a cooling globe, still highly heated within, the suggestion by Elie de Beaumont, in 1830, that "the inequality of cooling [between the interior and exterior portions] would place the crusts under the necessity of continually diminishing their capacities . . . in order that they should not cease to embrace their internal masses exactly," and that this condition "may with great probability completely account for the ridges and protuberances which have been formed on the external crust of the earth:"*—this seemed so natural and obvious an explanation, that it commanded a very general assent among geologists.†

* Phil. Mag., Oct., 1831, vol. x, pp. 263, 264.

† "Elie de Beaumont and some other geologists have attributed these effects and especially the elevation of mountains to the contraction of a cooling globe, and this appears to be the only one adequate for the results. . . . In attributing the plications of the earth's crust and the elevation of most mountains to a lateral pushing movement within the crust, there is nothing hypothetical. The statement is the expression simply of a fact. The conclusion that this tension [compression] is due to the contraction of a cooling globe has not yet been fully established. It is here adopted, because no other that is at all adequate has been presented."—*Prof. J. D. Dana, Manual of Geology*, 1863, part iv, chap. vi, sect. 1, pp. 721 and 725.

There were not wanting some however who questioned the sufficiency of the cause to accomplish the observed results; and our colleague Captain C. E. Dutton some ten or twelve years ago with great acuteness pointed out the entire inadequacy of the "Contractional Hypothesis," from an engineering stand-point.*

Mr. Robert Mallett, a zealous supporter of the contractional hypothesis, estimated that the diameter of the earth is now about 189 miles less than it was when entirely fluid.† That is, that the surface has collapsed ninety-four miles. Liberal as is this allowance (a surface contraction linearly of $\frac{1}{42}$), it would still be utterly insufficient to represent the actual average of compression. But in point of fact, the whole amount of cooling, and therefore of possible contraction by cooling, has been very much less than has generally been assumed.‡

1. No permanent crust could have been formed upon the molten globe until convective currents had well nigh ceased; that is, until the entire mass had cooled down very nearly (say —within a hundred degrees F.) to the congealing point.§

2. After the formation of a permanent crust, the greater part of the heat escaping through it would probably have been the "latent heat" from consolidation of new accessions to its inner surface, with little reduction of the sensible temperature of the inner mass.

3. Whatever *expansion* by freezing or consolidation of such accessions to the inner surface might occur, would to their extent counteract the effects of interior shrinkage.

4. The further escape of the encased heat through several miles of so poor a conductor as the earth's crust, must have since gone on with extreme and ever lagging slowness; and even through the lapse of a million centuries, must have been

* This Journal, 1874, vol. viii, pp. 113–123. The Rev. Osmond Fisher, in his Physics of the Earth's Crust (8vo, Lond., 1881), has also successfully attacked the hypothesis, chapter vii, pp. 73–75.

† Phil. Trans. Roy. Soc., June 20, 1872, vol. clxiii, pp. 147–227.

‡ "If we were to assign thirty miles as the diminution of the earth's mean radius since the first formation of a cooled exterior, we should probably reach the utmost limit consistent with Fourier's theorem."—*C. E. Dutton*, this Journal, 1874, vol. viii, p. 121.

§ Mr. William Hopkins, the author of the unfortunate "precession" argument against a fluid earth (or rather against a flexible earth-crust), in the same memoir, well remarks: "If the matter composing the globe was originally in a high state of fluidity from heat, the process of cooling would undoubtedly in the first instance be by circulation." And even supposing solidification to commence at the center, he maintains: "The superficial parts of the mass must in all cases cool the most rapidly; and now (in consequence of the imperfect fluidity) being no longer able to descend, a crust will be formed on the surface; from which the process of solidification will proceed far more rapidly downward, than upward on the solid nucleus."—*Phil. Trans. Roy. Soc.*, January 17, 1839, vol. cxxix, pp. 381–384.

largely compensated by the frictional heat continually generated by the tidal waves in the superior layers.*

Indeed we may venture the opinion that the ultimate refrigeration of a body like the earth must require a period of time whose curve would be almost hyperbolic to the asymptote; and that the entire solidification of a planet has not yet occurred in the solar system. Even our Moon, with a mass less than one-eightieth of our own, and with an exterior cooled probably below the freezing point of mercury, may still be very hot if not partially molten within.†

The volumetric contraction of our globe since the formation of its crust may therefore be regarded as practically very insignificant. On the other hand if we attempt to form an idea of the amount of average shortening of the strata in a given zone about the earth, from the amount of corrugation and displacement observed (assuming from the analogy of submarine contours that a corresponding degree of compression exists throughout the ocean beds), an allowance of one-eleventh, or of a primitive excess by one-tenth of the present girth, would probably not be regarded by the geologist as an undue assumption. Professor Alphonse Favre of Geneva in detailing some experiments to reproduce by lateral compression in layers of tenacious clay, the plications of strata, has estimated a linear contraction of one-third, as exhibited by portions of the Swiss Alps.‡ Our own Appalachians and western Sierras probably indicate as great an amount of shortening.

But an expansion of the circumference of a sphere by one-tenth, involves an increase of its volume of one-third; of course a wholly inadmissible assumption on any hypothesis of the formation of a crust. The conception of a gaseous or vaporous *intumescence* of a body like the earth may be dismissed at once, as simply opposed to physical possibility.§

* Mr. George H. Darwin has estimated that the internal heat developed by "the lengthening of the sidereal day from 5h. 36m. to 23h. 56m. would be sufficient (if applied all at once) to heat the whole mass about 3000° F., supposing the earth to have the specific heat of iron."—*Phil. Trans. Roy. Soc.*, Dec. 19, 1878, vol. clxx, pp. 495 and 535. And in a subsequent memoir, computing this past transformation of energy as equal to thirteen and a half times the whole remaining kinetic energy of the present rotation, he arrives at the somewhat startling conclusion: "Thus it appears that at the present rate of loss, the internal friction gives a supply of heat for 3560 million years." (Same volume, pp. 561 and 592.) He infers however that only about one-fiftieth of the present downward increase of temperature is referable to the past internal friction.

† This is of course in very strong antagonism to the conclusions reached by Sir William Thomson's elaborate calculations.

‡ La Nature, Sept. 28, 1878, vol. vi, part ii, pp. 278–283. A full abstract of the paper is given in Nature, Dec. 5, 1878, vol. xix, pp. 103–106.

§ This suggestion is broached by Mr. Fisher in his Physics of the Earth's Crust, and appears to be in some sense maintained in a recent German work (not seen by the present writer), on Terrestrial Physics (1884), by Von Siegmund Günther.

What solution of this seeming paradox have geologists to offer, after the demolition of the "contraction hypothesis?" As yet none has appeared. It would seem then that the problem must be attacked from a different quarter.

For a century it had been observed by astronomers that the moon's path exhibited a small acceleration (amounting to about 10″ or 11″ in a century), not accounted for by theory. Laplace investigating the problem, showed that as a result of the secular diminution of the earth's orbital eccentricity, there should be an acceleration of the moon of about 10″ in a century. And he announced it as established from a computation of the ancient eclipses that our "sidereal day has not changed by so much as one-hundredth of a second since the time of Hipparchus."*

That remarkable man, Dr. Julius R. Mayer of Heilbronn, in his very original essay on "Celestial Dynamics" published in 1848, showed that as the oscillations of the pendulum require for their maintenance a constant expenditure of power, so equally do the oscillations of the ocean tidal wave. "The moving waters rub against each other, against the shore, and against the atmosphere; and thus meeting constantly with resistance, would soon come to rest, if a *vis viva* did not exist, competent to overcome these obstacles. This *vis viva* is the rotation of the earth on its axis; and the diminution and final exhaustion thereof will be a consequence of such action. *The tidal wave causes a diminution of the velocity of the rotation of the earth.*" In the next chapter, discussing the consequence of Laplace's demonstration of the constancy of the length of the day in historic times, Mayer continued: "This result, as important as it was convenient for astronomy, was nevertheless of a nature to create some difficulties for the physicist. With apparently good reason it was concluded that if the velocity of rotation had remained constant, the volume of the earth could have undergone no change [by loss of heat]. . . . The earth's radius measures 6,369,800 meters, and therefore its length ought not to have diminished more than 15 centimeters [6 inches] in 25 centuries. . . . Considering what is known about the expansion and contraction of solids and liquids by heat and cold, we arrive at the conclusion that for a diminution of one degree [C.] in temperature, the linear contraction of the earth cannot well be less than the hundred-thousandth part. . . . The reason why in spite of this accelerating cause, the length of the day has nevertheless remained constant since the most ancient times, must be attributed to an opposite retarding action. This consists in the attraction of the sun and moon on the liquid parts of the earth's surface, as explained in the last chapter. According to the cal-

* Sir John Herschel's Outlines of Astronomy, chap. xviii, sec. 908. (See Laplace's Traité de Mécanique Céleste; tom. ii, liv. 5, and tom. iii, liv. 7.)

culations of this chapter, the retarding pressure of the tides against the earth's rotation, would cause—during the lapse of 2,500 years—a sidereal day to be lengthened to the extent of one-sixteenth of a second. As the length of the day however has remained constant, the cooling effect of the earth during the same period of time, must have shortened the day one-sixteenth of a second. A diminution of the earth's radius to the amount of $4\frac{1}{2}$ meters in 2,500 years, corresponds to this effect. Hence in the course of the last 25 centuries, the temperature of the whole mass of the earth must have decreased one-fourteenth of a degree [C.]."* In this ingenious adjustment, the eminent philosopher doubtless greatly over-estimated the earth's cooling and consequent contraction, in order to compromise the "difficulties of the physicist" with the authority of the astronomer.

In 1853, our colleague Mr. William Ferrel in a paper "On the effect of the Sun and Moon upon the rotatory motion of the Earth," independently undertook the mathematical investigation: the very name of Mayer being then (it is scarcely necessary to state) as wholly unknown in this country as it was in England. In this important paper it is shown that external forces of the second order—neglected by Laplace as producing no sensible effect upon the rotation period, are—though very small—decidedly appreciable, and "must, if not counteracted by some other effect, produce a sensible variation in the earth's rotatory motion." Assuming a lunar ocean tide of two feet, with a lag of two hours, Mr. Ferrel estimated an equatorial retardation of 37 miles per century, increased by the solar tide to 44 miles per century, which would give an apparent lunar acceleration of $1' 24''$ in a century. "As no such acceleration has been observed—above what is accounted for otherwise, the rotatory motion of the earth must be nearly uniform; and the above effect of the sun and moon must be accounted for by the gradual contraction of the earth through a loss of temperature." This would require a subsidence of the surface, or a reduction of the earth's radius (supposing the contraction equable through the mass), of about one foot per century.†

In the same year, a few months earlier, Mr. John C. Adams in a careful memoir "On the secular variation of the moon's

* L. E. D. Phil. Mag., May and June, 1863, vol. xxv, pp. 403, and 423–427. Also Am. Jour. Sci., 1864, vol. xxxviii, pp. 398, and 409–413.

† Gould's Astronom. Jour., Dec. 8, 1883, vol. iii, pp. 138–140. In this paper (p. 141), Mr. Ferrel shows that the similar retarding effect of the earth upon a primitive rotation of the moon (supposed originally fluid) must be 562 times greater than the converse earth retardation; and that this therefore sufficiently explains the present reduction of the moon's rotation to the period of a lunar month. This is believed to be the first suggestion of the now accepted cause of this peculiarity of the moon. Mr. Ferrel's paper is dated September, 1853. A similar explanation was offered by Helmholtz in the following year.

mean motion," pointed out an incompleteness in Laplace's investigation by the neglect of a certain series of terms; and he showed that the effect of this series would be to reduce the actual acceleration due to the diminution of eccentricity in the earth's orbit to about 6″; leaving the residual discrepancy of 4″ or 5″ of acceleration (as compared with the positions indicated from the ancient eclipses), to be still accounted for.*

Herman Helmholtz, in a lecture on the "Interaction of Natural Forces," delivered at Königsberg, Feb. 7, 1854 (without knowledge of Adams' work), pointed out to his hearers that "The motions of the tides, however, produce friction; all friction destroys *vis viva*, and the loss in this case can only affect the *vis viva* of the planetary system. We come thereby to the unavoidable conclusion that every tide—although with indefinite slowness, still with certainty—diminishes the store of mechanical force of the system; and as a consequence of this the rotation of the planets around their axes must become more slow."†

Charles Delaunay, who with fine analytic skill had for several years been engaged on the theory of the moon, in 1859 undertook a careful examination of the results obtained by Mr. Adams, which impeached the accuracy of his great countryman, Laplace; and in a memoir "On the secular acceleration of the mean movement of the Moon," presented to the French Academy, he fully confirmed the bold criticism of Mr. Adams.‡ And he affirmed that the necessary elongation of the day by tidal retardation is quite competent to account for the apparent residual acceleration of the moon.

In 1862 the eminent mathematician, Arthur Cayley, independently investigated the problem; and in a paper "On the secular acceleration of the Moon's mean motion," he also corroborated the correctness of Mr. Adams' great discovery.§

In 1864, eleven years after his first suggestive memoir, Mr. Ferrel returned to the subject of the elongation of our day. In a "Note on the influence of the Tides in causing an apparent secular Acceleration of the Moon's mean motion," he remarks: "At the time of the original publication of Mayer's paper, and also at the time of my own, Laplace's result of

* Phil. Trans. Roy. Soc., June 16, 1853, vol. cxliii, pp. 397–406. Also Monthly Notices, Roy. Astr. Soc., 1853, vol. xiv, pp. 59–62. And Reply to Objections, M. N., 1860, vol. xx, pp. 225–240, and 279–280.

† L. E. D. Phil. Mag., June, 1856, vol. xi, p. 513. Also Am. Jour. Sci., 1857, vol. xxiv, p. 212.

‡ Comptes Rendus Acad. Sci., Jan. 17 and April 25, 1859, vol. xlviii, pp. 137, 138, and 817–827. Also numerous papers on the subject in succeeding volumes. Especially a paper "On the apparent acceleration of the mean motion of the Moon, due to the actions of the Sun and Moon on the waters of the sea."—Comptes Rendus, 1866, vol. xlii, pp. 197–200, 575–579, 704–707, etc.

§ Monthly Notices, Roy. Astr. Soc., 1862, vol. xxii, pp. 171–230.

about 11′ per century for the amount of acceleration of the moon's mean motion arising from a secular change of the eccentricity of the earth's orbit was supposed to be correct; but since that time it is known that Adams and Delaunay, and also quite recently, Mr. Cayley, have all obtained less than 6″ for the amount of secular acceleration per century. This determination, compared with the most reliable determination of the acceleration deduced from the discussion of the recorded observations of ancient eclipses, leaves about 6″ to be accounted for by tidal action or some other *unknown cause.*" He then proceeds to state that Mayer's hypothesis of a one-and-a-quarter hour retardation, and his own of a two-hour retardation of the tidal wave, "are both much too great to be reasonable hypotheses, or to be necessary to account for the balance of the moon's secular acceleration over and above the late determination." And he concludes "that tidal action is adequate to account for 6″ of secular acceleration upon a very reasonable and probable hypothesis with regard to the magnitude and retardation of the tidal wave by friction after making due allowance for the effect of a probable contraction of the earth's volume."*

In 1867, Prof. William Thomson, discussing the same subject says: "The tides must tend to diminish the angular velocity of the earth's rotation, and this tendency is not counterbalanced to more than a very minute degree by the tendency to acceleration which results from secular cooling and shrinkage."†

This brief historic summary has been introduced to emphasize the strengthening conviction among physicists of the necessary variability of that standard unit of time, which to the astronomers had seemed the most permanent constant of our solar system. And these references serve to establish beyond a reasonable doubt the remarkable fact that our present day is considerably longer than the day of early geologic times. For nearly two centuries has the moon been seen in advance of its periodic position—to the small amount (as estimated from ancient eclipses), of about ten or eleven seconds of angle per century. Of this relative displacement about one-half is now known to be due to the actual falling behind of the terrestrial observer, and the other half to Laplace's great discovery—the influence of the diminishing eccentricity of the telluric orbit.‡

Mr. George H. Darwin (as is well known) has in recent years been engaged in elaborate investigations of this subject, and he has drawn some important consequences from his discussion.

* Proceedings Am. Acad. Arts and Sci., Dec. 13, 1864, vol. vi, pp. 379–383.

† Thomson and Tait's Natural Philosophy, 1867, vol. i, Sec. 830, p. 686.

‡ An influence which must reach its inferior limit in about one precession period.

He says: "As we look at the whole series of changes from the remote past, the ellipticity of figure of the earth must have been continually diminishing, and thus the polar regions must have been ever rising, and the equatorial ones falling; but as the ocean always followed these changes they might quite well have left no geological traces. The tides must have been very much more frequent and much larger, and accordingly the rate of oceanic denudation much accelerated. The more rapid alternations of day and night would probably lead to more sudden and violent storms, and the increased rotation of the earth would augment the violence of the trade winds, which in their turn would affect oceanic currents. Thus there would result an acceleration of geological action."*

In this suggestive retrospect one important physical condition, however—which probably *has* "left its geological traces" on our globe—does not appear to have attracted the author's attention. A shell formed upon the spinning earth when its oblateness was considerably greater than at present obviously could not fit the spheroid as the ellipticity of its meridians diminished. Not only would the crust be quite sensibly too large, as a whole, but especially would its equatorial girth have to be notably reduced.

If in imagination we may carry back the formation of a consistent crust (of some few miles thickness) to an epoch when the rotation of our planet was at four times its present rate—that is, when the day measured but six of our hours,—the equatorial radius (assuming a true ellipsoid of revolution, and neglecting the small amount of contraction by loss of heat), would have been about one-tenth greater than it now is, or 4359 miles; and the polar radius about one-sixth less, or 3291 miles. In other words, the poles would have been about 658 miles nearer the center of the earth than they are at present, and the equatorial protuberance about 396 miles higher than at present.

With an equatorial shell one-tenth greater in circumference than the present dimensions, it is evident that from the very slow but never ceasing contraction due to diminution of rotatory motion, this crust would be subject to an unremitting stress of lateral compression as relentless as that from the old hypothetic shrinkage of volume by reduction of temperature. Is it not precisely this *morphologic* contraction whose effects and records are everywhere apparent in the crumpling of the earth's crust? Here is a true cause: here is a sufficient cause: here is a necessary cause—so importunate that it cannot readily be dispensed with or explained away.

An objection to this hypothesis, based on the supposed

* Phil. Trans. Roy. Soc., December 19, 1878, vol. clxx, p. 532.

solidification of our planet since its present form has been reached, requires a notice here, rather from the character of its source, than from its own intrinsic force or importance.

Professor William Thomson, adopting Mr. Adams' estimate of tidal retardation, thus argues: "If the rate of retardation had been uniform since ten million centuries back, the earth must have been then rotating faster by one-seventh than at present, and the centrifugal force greater in the proportion of 64 to 49. If the consolidation took place then, or earlier, the ellipticity of the upper layers of equal density must have been $\frac{1}{230}$ instead of about $\frac{1}{300}$ as it certainly now is. It is impossible to escape the conclusion that the date of the consolidation is considerably more recent than a thousand million years ago.*

Professor Peter G. Tait (as if not to be outdone), is still more emphatic and positive in his enunciation. He says: "It being thus established that the rate of rotation of the earth is constantly becoming slower the question comes: How long ago must it have solidified in order that it might have the particular amount of polar flattening which it shows at present? Suppose, for instance, it had not consolidated less than a thousand million years ago. Calculation shows us that at that time, on the most moderate computation, it must have been rotating at least twice as fast as it is now rotating. That is to say, the day must have been 12 hours long instead of 24. Now if that had been the case, and the earth still fluid throughout, or even pasty, that double rate of rotation would have produced four times as great centrifugal force at the equator as at present, and the flattening of the earth at the poles and the bulging at the equator would have been much greater than we find them to be. We say then, that because the earth is so little flattened it must have been rotating at very nearly the same rate as it is now rotating, when it became solid. Therefore, as the rate of rotation is undoubtedly becoming slower and slower, it cannot have been many millions of years back when it became solid, else it would have solidified into something very much flatter than we find it."†

The supposition that a granite mountain or equatorial protuberance 400 miles high or 100 miles high, could for a moment support itself would hardly be entertained by a practical civil engineer.‡

It is now nearly forty years since Herbert Spencer, with a juster physical insight, contended and satisfactorily showed that a solid earth (of any shape) would assume the oblate

* Nat. Philos., 1867, vol. i, Sec. 830, p. 687.

† Lect. on recent advances in Phys. Sci., 1876, lect. vii, pp. 173, 174.

‡ The limiting modulus of height of a granite pyramid (equaling one side of its square base), is somewhat less than eleven miles.

spheroidal form due to its rate of rotation, as certainly and promptly as if it were liquid.*

Granting then that the earth were solidified with an oblateness such as has just been assumed, its present dimensions—and probably its present topography—would not be very different from what we now find them.

But another very serious error in the estimates of both these quotations should not be overlooked. While it is quite impossible to calculate the time when the equatorial radius was one-tenth greater than at present, it is very certain that any estimates based on the existing retardation from the ocean tides, must be utterly fallacious. Not only has this tidal action been continually decreasing (by some small exponent) from the earliest times, but the bodily tides of the liquid or solid earth have undoubtedly played a far greater part than the superficial ocean tides—in the absorption and destruction of its rotatory momentum.† It is not at all probable therefore that it has required "a thousand million years" to effect the present equatorial contraction of one-eleventh from the former diameter of 8,718 miles.

The much debated question of the probable degree of rigidity of our planet is therefore quite irrelevant to the problem before us. And yet with so much confidence and persistence has the pet-hypothesis of its entire solidity been maintained,—the temptation is strong to waste upon it a collateral glance.

Professor William Thomson contends: "Had the solid part of the earth so little rigidity as to allow it to yield in its own figure very nearly as much as if it were fluid, there would be very nearly nothing of what we call tides, that is to say, rise and fall of the sea relatively to the land; but sea and land together would rise and fall a few feet every twelve lunar hours. This would (as we shall see) be the case if the geological hypothesis of a thin crust were true. The actual phenomena

* "However great in a given portion of matter may be the excess of the form-preserving force over the form-destroying force, it is clear that if during augmentation of bulk the form-preserving force increases only as the *squares* of the dimensions, while the form-destroying force increases as their *cubes*, the first must in time be overtaken and exceeded by the last; and when this occurs the matter will be fractured and re-arranged in obedience to the form-destroying force."—*Herbert Spencer*. And the author estimates that "the most tenacious substance with which we are acquainted—when subjected to the same forces that are acting upon the earth's crust—would exceed the limit of self-support determined by the above law, before it attained the thousand-millionth of the earth's bulk."—*L. E. D. Phil. Mag.*, March, 1847, vol. xxx, pp. 194–196.

† Mr. G. H. Darwin remarks: "Whatever may be thought of the theory of the viscosity of the earth and of the large speculations to which it has given rise, the fact remains that nearly all the effects which have been attributed to the action of bodily tides would also follow (though probably at a less rapid rate), from the influence of oceanic tides on a rigid nucleus."—*Phil. Trans. Roy. Soc.*, Dec. 19, 1878: vol. clxx, p. 538.

of tides therefore give a secure contradiction to that hypothesis."*

That a siliceous crust of 20 miles average thickness, and an overlying aqueous ocean of three miles average depth, should have (as required by the argument) so equal a coefficient of mobility, that sea and land could thus "together rise and fall," might well be pronounced incredible. Professor Thomson himself proceeds to very seriously damage his "secure contradiction" of "the geological hypothesis," by adding immediately after the passage just quoted: "We shall see indeed presently that even a continuous solid globe of the same mass and diameter as the earth would if homogeneous and of the same rigidity as glass, or as steel, yield in its shape to the tidal influence, three-fifths as much, or one-third as much, as a perfectly fluid globe."†

When we have learned the elements of depth, inertia and fluidity of the disturbed layers beneath the geologic crust, then and not till then may the beginnings of a mathematical theory of the telluric tides be attempted; and some plausible measure of their "lagging" be suggested. But in our profound and insuperable ignorance of these interior conditions determining the magnitude and direction of the tidal crests, it is idle for the mathematician (with whatever array of formidable differential equations) to gravely assure us that "no very considerable portion of the interior of the earth can even distantly approach the fluid state."‡

It is in fact quite incontrovertible, that whether the geologic crust have for its content a glowing lava, or Thomson's ideal steel, it is equally subject to lunar and solar tides fully comparable to the more apparent ocean tides. This last surviving argument for solidity, from the theory of the tides, should therefore be dismissed as being no less futile than the abandoned argument, from the theory of precession.§

* Nat. Philos., 1867, vol. i, sect. 833, p. 690. Retained in the new edition.

† Same work; sect. 833, pp. 690, 691, and sect. 834, pp. 691–694.

‡ Mr. George H. Darwin in a very abstract memoir: "On the bodily tides of viscous and semi-elastic spheroids, and on the Ocean Tides upon a yielding nucleus," read before the Royal Society of London, May 23, 1878, arrives at the conclusion: "Unless the viscosity were very much larger than that of pitch, the viscous sphere would comport itself sensibly like a perfect fluid, and the ocean tides would be quite insignificant. It follows therefore that no very considerable portion of the interior of the earth can even distantly approach the fluid state."—*Phil. Trans. Roy. Soc.*, vol. clxx, p. 28.

§ Professor Thomson, who in 1862 had re-enforced and exaggerated the Hopkins argument, by maintaining that the earth's defect (by deformation) from the theoretical amount of precession, is "much smaller for instance than it would be if its effective rigidity were no more than the rigidity of steel," in consequence of an oral discussion with our colleague Professor S. Newcomb, in 1876, with a frankness worthy of all praise, made a full surrender of the position, in his address before the mathematical and physical section of the British Association: Report Brit. Assoc., Sept., 1876, vol. xlvi, part II, pp. 1–12), and has since con-

Returning to the consideration of the varying oblateness of the terrestrial ellipsoid, and its geological consequences, we naturally inquire what indications remain to corroborate or to impair our hypothesis. Could we ascertain the approximate amount of lateral excess of shell by its corrugations, say for each parallel zone of ten degrees, these enlarged circumferences divided by the cosine of the latitude would give us the variations (if any) of envelope, throughout a given meridian. This is of course entirely beyond our reach. We may, however, infer that the altitudes of mountain ranges should bear some rude proportion to the general amount of crumpling in neighboring districts; and that at least some approximate indication might thus be afforded or suggested. We might expect that circumpolar regions should be free from mountains or plications; and that the inter-tropical region should contain the highest elevations.

Dr. Arnold Guyot, in his excellent summary of the Physics of the Globe, prepared for Johnson's Cyclopædia, gives the following characteristic sketch: "In the New World therefore the highest lands are piled up in the southwest, in the Old World in the southeast. . . . On the whole, the reliefs begin with the vast low plains around the polar circle, and go on increasing from the shores of the Arctic Ocean toward the tropical regions. The highest elevations however are not found at the equator, but north of the Tropic of Cancer in the Old World, in the Himalayas 28° N., and north of the Tropic of Capricorn in the New World, in the Andes of Bolivia 16° S."*

The same eminent and conscientious physiographer, in his paper on the Ocean, remarks: "On the whole, the ocean beds become less deep toward the north pole, just as the lands become lower toward the same region."†

In this significant epitome—presented certainly with no view of supporting an hypothesis, we have perhaps as striking a suggestion of greater oblateness in former times as we could expect to find preserved to us. The curious circumstance that the highest lands on either hemisphere are found much nearer to the Tropics than to the Equator, it must be admitted, is difficult of explanation. Theoretically we should expect no sensible differences throughout the inter-tropic zone. We may however remember that the tides of the comparatively thin and yielding crust exert their greatest influence on the tropical zones; being for the larger portion of the year very near to

ceded "that a perfectly fluid spheroid has a precession scarcely differing from that of a perfectly rigid one."—Mr. Darwin, in *Phil. Trans. Roy. Soc.*, Dec. 19, 1878, vol. clxx, p. 525.

* Johnson's Cyclopædia, 1875: art. "Earth," vol. i, p. 1455.

† Same work, 1877, art. "Ocean," vol. iii, p. 918.

those circles; while the equatorial tides—during the equinoctial periods—occur when the sun (and the mean lunar plane) have their most rapid change of declination. If it be said that a tidal elevation of two or three feet is too utterly insignificant a space, and a cycle of half a year too utterly insignificant a time to affect the vast erections of secular progression, the reply is that this minute relaxation of particular zones—unceasingly repeated and continued—may well facilitate the slow but gathering movements of the crust from its strong compression, and may even to some extent give direction to their energies. As a concurrent though perhaps unimportant circumstance, the fact may be mentioned that while the northern tropic encounters 35 per cent of dry land, and the southern tropic nearly 25 per cent, the equator embraces but 21 per cent of land.

George Darwin, discussing the differences of tidal energy and lagging at different latitudes (in a viscous spheroid), by reason of which the retarding influence is greatest at the equatorial zone, suggests the possibility of this tendency causing a westward drift of mountain folds and continents in the lower latitudes (as compared with polar regions) and remarks: "There can be little doubt that on the whole, the highest mountains are equatorial, and that the general trend of the great continents is north and south in those regions. The theoretical directions of coast line are not so well marked in parts removed from the equator."* Here again, the striking significance of this physiographic generalization appears to be strangely overlooked.

It has been an occasion of some surprise that in all the geological literature to which I have had access, I have been able to find but a single allusion to the hypothesis here advanced in explanation of the palpable tangential compression of the earth's exterior strata.

This one allusion is given in Mr. O. Fisher's interesting work on the "Physics of the Earth's Crust," and is by him referred to, only to be dismissed in a brief paragraph. His reference is as follows: "The friction of the tides—whether oceanic or bodily—must necessarily have diminished the rotational velocity, and lessened the oblateness. The parts of the crust about the poles will consequently have been subjected to stretching, and those about the equator to compression. There is however no apparent reason immediately to connect the inequalities with this cause, for the continents do not occupy an equatorial belt—as they would do under this hypothesis, nor have the polar regions been free from the compression which all continental areas have experienced."†

* Phil. Trans. Roy. Soc., Dec. 19, 1878, vol. clxx, p. 589.

† Physics of the Earth's Crust, 8vo, Lond., 1881, chap. xiv, p. 183.

The expansion or stretching of the polar regions—referred to, though probably less than Mr. Fisher would imagine, would be attended with no dislocations, and would leave no traces. A crust of ten or twenty miles depth pressing upon its interior bed with a weight of five thousand or ten thousand tons to the square foot, would flow (excepting its exterior film of a mile or two),—on any relief of lateral pressure—as quickly and as uniformly as so much plastic clay.

To Mr. Fisher therefore belongs (so far as I am aware) the credit of the first suggestion of a possible connection between variation of oblateness and mountain-building;—even though by him discarded. He does indeed on the following page recur to the topic; and admitting that this variation *ought* to have produced some appreciable effect, he suggests that this apparent want of relation between the occurrence and the phenomena, favors the idea of a change of latitudes.

So strongly impressed is the writer with the inevitable operation and potency of this unquestioned retardation of rotation, that were all traces of any differential action masked and obliterated, he would still hold to it as the one efficient cause (alone—as yet suggested) to account for the prominent constriction of the crust—displayed in every land. But the differential traces of oblateness have *not* been obliterated;—masked though they may be to some extent, by other perturbations.

The suggestion of a change of axis, is one which will be entertained by the physicist with extreme hesitancy and caution. Professor William Thomson indeed has stated that "the axis of maximum inertia, and axis of rotation—always very near one another—may have been in ancient times very far from their present geographical position; and may have gradually shifted through 10, 20, 30, 40 or more degrees, without at any time any perceptible sudden disturbance of either land or water."* George Darwin also has admitted that if the earth be not solid, "As in successive periods the continents may have risen and fallen, the pole may have worked its way in a devious course some 10° or 15° away from its geographical position at consolidation; or may have made an excursion of smaller amount, and have returned to its old position."†

That under the continued stresses of precession and nutation—there should have been a slight slipping of the crust on its fluid interior, is not improbable. The poles of the shell in such case might describe a small spiral about the "axis of maximum inertia," but could probably never diverge therefrom more than a very few degrees. Nor could such gyration

* Report Brit. Assoc., 1876, vol. xlvi, part ii, p. 11.

† "On the Influence of Geological changes on the Earth's Axis of Rotation." Phil. Trans. Roy. Soc., Nov. 23, 1876, vol. clxvii, p. 305.

in any degree affect the sidereal axis of rotation—or its angle of obliquity with the plane of the ecliptic. Any such *geographical* change of the axis therefore as admitted by Thomson and Darwin, could only be a shifting of the mass of the earth (so to speak) upon an axis fixed in angular direction, with a corresponding shifting within its substance of the equatorial plane of oblateness.

Were we at liberty to imagine a translocation of the northern geographical pole—in the remote past, as far as to the arctic circle near Bering's Straits, the equatorial region would nearly correspond with Guyot's line of demarkation of the three northern—from the three southern continents.* This re-arrangement would also bring the highest portions of the South American Andes and of the Asiatic Himalayas much nearer to the equator, and the remarkable elevations of southern Europe—the Pyrenees, the Alps and the Caucasus (now all near the 45th degree of latitude), to the northern tropic. This, however, is mere speculation, for which no scientific warrant can at present be given.

An objection urged with considerable force by Captain Dutton against the hypothesis of the earth's contraction through secular cooling, should not be overlooked since it lies equally against the supposition here presented, and indeed against any hypothesis of general contraction. He remarks: "The determination of plications to particular localities presents difficulties in the way of the contractional hypothesis which have been underrated. . . . . The tendency of corrugation to occur mainly along certain belts with series of parallel folds, is not explained by assuming that these localities are regions of weakness. For a shrinkage of the nucleus would throw each elementary portion of the crust into a state of strain by the action of forces in all directions within its own tangent plane. . . . . The plications of the Paleozoic rocks do not conform, either in Europe or America, to the consequences here affirmed. These disturbances are localized in long and rather narrow belts, and if they truly represent contraction on certain great circles, then such contraction must have been enormous in arcs perpendicular to the axes of plication, and very little in arcs parallel thereto. Still more discordant is the contractional hypothesis with the Tertiary plications. From Cape Horn to the Bering's Sea is a continuous belt, very narrow for most of the distance, but extremely disturbed throughout."†

* Professor Guyot, referring to the general direction of the line of separation marked out by the Caribbean and Mediterranean Seas, with their adjacent isthmuses, remarks: "These regions are parts of a broad transverse band whose position can be traced from Bering's Straits as a center, with a meridian arc of 80° radius, and which we would call the *central zone of fracture.*"—Johnson's Cyclopædia, 1875, vol. i, pp. 1449, 1450.

† Am. Jour. Sci., August, 1874, vol. viii, pp. 121, 122.

It cannot be denied that the difficulty here set forth is a very puzzling one. Especially inexplicable appears the instance last referred to—of an unbroken range of mountain foldings extending over more than a third of the globe's circumference. While the *force* at the command of the rotating planet is abundantly sufficient to accomplish the result evidently some supplementary considerations are requsite to give the observed *direction* to this force. Beyond all question, the surface of the earth *has* been subjected to a compressing stress of tremendous energy. To account for the stress is one problem; to account for its resultants is another and probably much more complicated one.

The mere mechanical difficulty, however, of transmitting stresses through comparatively undisturbed areas of hundreds of miles of a flexible, friable, and practically plastic crust—with a large coefficient of viscous friction beneath—is not so formidable as might at first sight appear. It must be borne in mind that the pressures derived from an action so slow as from century to century to be scarcely sensible, are of an order of very great intensity, but of very small quantity. Under the continued urgency of rapidly revolving tidal waves (though also of a very minute order), there does not seem any improbability in the supposition that with the long time element such stresses may gradually be equalized or transmitted by what might be termed a process of "conduction" almost indefinitely.

From various considerations we may infer that in all geological ages the progress of elevation has been in excess of that of degradation by erosion; that in all ages mountain building has been at a maximum; that is, the uplifted heights have been the greatest which the average thickness of the crust at the time was capable of supporting; so that the former has been a constant function of the latter, the ratio being probably not far from one-fifth.

We may also infer that this increasing maximum of elevation must now have practically reached its limit, since both the processes of equatorial contraction and of internal temperature reduction are going on with extreme and lengthening slowness; and the whole remaining subsidence of the inter-tropical oblateness cannot exceed five miles, during the vast ages in which the earth's rotation shall be entirely arrested.

Looking back through the long vista of the unmeasured past we behold in imagination our planet in its early youth, with its expanded tropical surface as yet unmarked by wrinkles, endowed with superfluity of rotary activity, and subject to far more energetic tides and winds and tempests than we know at present. Its primitive and lowly organisms with as yet unspecialized sensations, accustomed to a dusky day of a few brief hours succeeded by as short a night. The moon—perhaps much nearer and of ampler but paler disk—speeding through its phases in the quick period of a week or less. The sun—of enormously greater volume than we behold it—diffusing its hazy beams, and probably expending a much smaller amount of radiant light and heat upon the flat and dreary face of nature than it now does after many hundred million years of waste.* Such is the outline of early meteorological conditions which the geologist should take into his account when theorizing on the grand dynamics of his science.

But of that primeval period of fleeting days and of shortened axis, perhaps the only physical record and memorial left us is the wide array of distorted crumplings and ruptured foldings (culminating apparently in the lower or middle latitudes) which have formed the needful condition and environment for man's advancement, and which have never ceased to excite the wonder and admiration of his observant and inquiring intelligence.